THE
MATHEMATICAL GAZETTE.

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THE MATHEMATICAL ASSOCIATION have undertaken to organise an exhibit, illustrative of Mathematical Education in Great Britain, for the Educational Section of the forthcoming International Congress of Mathematicians, to be held at Cambridge in 1912. It is specially desired to obtain specimens of ordinary mathematical work, both good and average, in various types of schools, as well as models and diagrams. All those willing to co-operate are requested to communicate with the Hon. Secretary of the Exhibition Committee, P. ABBOTT, Esq., Wildwood, West View, Highgate.

THE THEORY OF ORDER, AS DEFINED BY BOUNDARIES.

I. INTRODUCTION.

I PROPOSE in these papers to demonstrate, or rather to indicate the nature of the demonstration of, the proposition that—*All Geometry, whether projective or metrical, may logically be regarded as merely a particular concrete application of the general theory of Order, as defined by Boundaries.*

It is true that Geometry, as ordinarily understood, is something more than this; it is concerned with the actual subjective conceptions we entertain of Space, and so far provides matter of discussion to the Psychologist; and it is concerned with the actual objective measurements we make upon physical bodies in Space, and so far concerns the Physicist; while in both these respects it extends beyond the region of pure Logic into those of Epistemology and Philosophy generally. But so far as the pure Mathematician is concerned, every geometrical theorem is embraced in the Theory about to be discussed.

This Theory divides itself naturally into three parts:

(i) The Cataloguing of Continuous Groups;
(ii) Collations in the Catalogue;
(iii) Theorems deducible from the above.

The first of these corresponds to the geometrical problem, how to determine a system of co-ordinate axes, but without assuming to begin with any of those spatial conceptions upon which the definitions of Geometry are ordinarily based. As the name I have given it implies, the problem is essentially that of giving to the units of thought names by which we may be enabled logically (*i.e.* by means of *words*) to discuss their Order relations. This is done, briefly, by reducing groups of the order n to groups of the first order of groups of the order $(n-1)$, and so reducing the problem of Order in general to that of serial, or one-dimensional order.

And it is with this latter problem that I deal in these papers, under the title *Collations in the Catalogue*. After this there remain, of course, all the problems of Geometry to be dealt with, upon some of which the Theory of Order is capable of throwing some interesting light. I do not, however, intend to discuss any of these problems here, nor shall I say more about those in part (i) than is necessary to make clear the definitions which are required for part (ii), which I proceed to give, in rather discursive form, indicating the words to be defined by printing them in *italics*.

Suppose we are given an assemblage of objects, or *units of thought*, which, in accordance with some convention, or *rule of contiguity*, may be thought of successively, or as I say, *passed in review*, in such wise that we are bound (in theory), after thinking of any one unit, to think next only of one of a certain sub-group of units which are said to be *contiguous* to it, and not of any other unit chosen at random in the assemblage. Such an assemblage, which may be so passed in review, I call an *Ordered Group*.

The relation of contiguity discussed here is a reciprocal one, *i.e.* if A is contiguous to B, then B is also contiguous to A. But unless the contrary is expressly stated, I assume that in "passing in review," after passing from A to B, we pass on from B to some new unit, and not at once back to A again. In general, therefore, an unit has at least two units contiguous to it, even in the simplest ordered group. Such a group, a serial group, or *group of the first order*, might be defined by saying that its rule of contiguity is that each unit has two, and only two, units contiguous to it, but such a definition could not be extended to higher groups, nor does the full significance of such a definition appear except in connection with the conception of a Boundary, which I define in general terms as follows:

A *Boundary* is a sub-group contained in a wider group of units, which divides the whole into two distinct parts, in such a way that—

(i) it is impossible to "pass in review" from any unit in one of the parts to any unit in the other, without passing at least one unit of the boundary on the way, and

(ii) every unit in the boundary is contiguous to at least one unit in each of the two parts; so that it is not necessary to pass in review more than one of the units in the boundary.

A boundary is in general an ordered group, and is said to be *of an order* less by one than that of the group it divides. Two units respectively in the two parts into which a boundary divides a group are said to be separated by the *boundary*, which is said to be *between* the units. In general, therefore, the units contiguous to a given unit, since they form a boundary separating it from all other units, form an ordered group. In the case of a group of the first order, however, this is not the case, since the two units cannot by themselves be passed in review. We may, however, speak of them as forming a boundary of the order zero.

If we pass in review units of a group of order higher than the first, the units passed in review form a sub-group of the whole assemblage, which is of the first order as far as any part of it excepting the first and last units are concerned. If, however, the first and last units are the same, and no unit was passed in review twice, then the sub-group is a complete example of a group of the first order, in which any two units, not contiguous to one another, form a boundary. It is an *uniform* group, *i.e.* the rule of contiguity is the same at every unit in it. If the first and last units are not the same, the rule of contiguity is not the same for them, for each is contiguous to only one unity, instead of two, as in the case of the other units. Such a group may be called a *terminated* one, and the first and last units its *terminal* units.

Unless otherwise stated, when I speak of groups, I shall mean ordered, uniform, continuous, isomorphic group. I use the word *continuous* in its strict mathematical sense; by *isomorphic* I mean that it is such that the two parts into which any boundary divides the whole (except the limiting case of the boundary formed by the units contiguous to an unit, or to those of a sub-group of order less than that of a boundary group) are from the point of view of Order indistinguishable from one another. Terminated groups occur only as parts of complete groups, or in a special sense which does not really violate this definition.

(1) I may, therefore, lay it down as a general proposition that—*In any group of the first order a single unit cannot form a boundary*, or be said to be *between* two others.

I do not propose to go at length into the question of how a continuous group, even of the first order, can be catalogued, nor of the mathematical theory of continuity in connection with this problem, though these questions might well repay investigation at another time. It is sufficient for my present purpose to employ the twenty-six letters of the alphabet as a catalogue; but as the groups to be catalogued are uniform, and their order is defined solely by boundaries, we must be prepared to "pass the alphabet in review" indifferently forwards or backwards, and, moreover, we must pass on from Z to A, or from A to Z, without pause, so to speak. With these conventions the alphabet is a perfect example of an uniform, albeit discrete, group of units of thought, of the first order; though, not being "isomorphic," there would be difficulties in applying a similar catalogue without reservations to groups of higher orders. And in applying it even to groups of the first order, it must be borne in mind that two letters which are consecutive in the alphabet are not to be taken to represent contiguous units. Indeed, the great advantage of defining Order by means of Boundaries is that it is not necessary to talk about contiguous units, except very rarely, and then only in limiting cases of general propositions.

In naming the units of a given group of the first order from the catalogue, any name, selected absolutely at random, may be given to the first unit. Suppose we choose K. For the second unit of the group we may similarly choose at random any name, *except* K. So also for the third unit we may choose at random any name but those already chosen. For we know nothing about, and cannot possibly say anything about, the order of the three units of thought, except that the units are different from one another. It takes at least two units, even in a group of the first order, to form a boundary, and two more to be separated by that boundary, so that four is the smallest number of units of thought with respect to which it is ever possible to predicate any order relation whatever. It is, therefore, only when we come to choose the fourth and subsequent names that the order of the units imposes any restriction upon our freedom of choice. Thus, if the first three names chosen were

K, *B*, and *P*, and we find the fourth unit is separated from the first by the boundary formed by the second and third, we cannot choose, for example, *G*, nor *M*, for neither of these is separated from *K* by the boundary (*BP*). But we might call the fourth unit, say, *T*, or *Y*, or *A*. In the latter case, it is true, we might be bothered to find a name for a subsequent unit, separated from the first and third units by the second and fourth, but this difficulty, which is only due to the inadequacy of our discrete catalogue, can easily be removed, though it is not necessary to go into the matter here. I have already taken what may seem unnecessary trouble about a very simple matter, but it will be found to be of the utmost importance that the amount of freedom of choice remaining at each stage of the process of naming units from the catalogue should be exactly appreciated.

To compare the orders of units of thought in two different groups, we may pass them in review together, thinking of one unit from each group at the same time, and so *collating* the two units, and when all the units of both groups have been passed in review, the two groups may be said to have been collated, or a *collation* effected between them. This assumes that the passing in review of both groups begins and ends with one and the same pair of collated units, and that no unit in either group is passed more than once. This will always be assumed to be the case here, and it may be observed that this condition is automatically fulfilled if we effect the collation indirectly, by naming the units of both groups from the same catalogue, for by so doing each unit in the one group is collated with one, and only one, unit in the other, namely, the one which receives the same name. We may in this way effect collations of the units in one group with themselves by simply re-naming the group. But at the same time such a re-naming may equally well be regarded as collating together the two names successively given to each unit, and so may be regarded as a "collation in the catalogue." It is in this light that I prefer to regard such collations, which form the subject matter now to be investigated.

II. Collations in the Catalogue.

In order to facilitate the discussion I shall represent Collations by what I call *Schemes*. In these, the catalogue names of units of a group (for which I always use capital letters), or symbols (small letters, etc.) representing them, if the catalogue names are not known, are written in one line, and the new names into which they are changed by the collation immediately below them on the next line. Where the order of the units is known, *e.g.* if catalogue names are employed, they should be written down in order on the lines. But this is only for convenience; the arguments deduced by means of the schemes do not depend in any way upon the order in which the letters are written down. The only data expressed by the schemes are the collations which are represented by placing two letters in the same vertical column.

In order to distinguish between collations which are given by the data of the problem and those which are deduced from them in the course of the proof, I put letters added in the scheme in virtue of such deductions between brackets.

If the conditions of the problem would admit of the determination of any name, if it were required, it may be represented by a +. But if the conditions of the problem determine that any letter whatever might be written in a given place, I represent it by the "indeterminate symbol," thus... The significance of this will appear later.

When writing about the names in a scheme, a numerical suffix may be used to denote to which line of the scheme the name belongs.

Thus in the scheme—

$$\left.\begin{matrix} A & B & K & (P) & X & (x) \\ B & x & L & + & (P) & \ldots \end{matrix}\right\}, \qquad\ldots\ldots\ldots\ldots(a)$$

the catalogue names A_1 and B_2 are collated together, while B_1 is collated with some catalogue name which is represented by the small letter x_2; K_1 is collated with L_2. These collations constitute the data of the problem, and from them we deduce that a particular letter, which it is not necessary to name, is collated with P_1, while X_1 is collated with P_2, and x_1 is collated indeterminately.

It must be remembered that schemes do not prove things by themselves; they are only graphic aids to the mental process of passing in review; they only help to make the proofs *anschaulich*. The essence of the proofs always lies in the inferences as to boundaries, *i.e.* whether one can, or can not, pass in review without passing certain units, or one of them, on the way. Now it is true that passing in review is a process in time, and as such depends on an asymmetrical relation; it has sense; and in passing in review a group of the first order, I shall assume that we pass always in the same sense till the whole group has been passed. It is, however, easy to define sense by means of boundaries alone, provided we are referred to not less than three given units in the group, without assuming any asymmetrical relation between them; and moreover, without discriminating between contiguous units, and so introducing the difficulties involved in the mathematical conception of continuity. This, therefore, I proceed to do.

When passing in review a group of the first order, the initial unit, that is the first one thought of, and the unit under review at any moment constitute a boundary dividing the whole group into two parts, the one containing all the units already passed in review, and the other all those which have yet to be passed. This condition ensures that the sense of the passing in review shall be the same throughout.

Again, if we pass the same group in review twice, or collate a group of the first order with itself, the question will arise whether the senses in which we passed were the same, or different. More particularly it will be seen that the nature of collations in the catalogue depends essentially on whether the names under review pass in the same or in opposite senses, a distinction which may be defined as follows:

When passing in review a collation in the catalogue, if *initially*, *i.e.* before either of the pair of collated names passes the boundary in the catalogue formed by the first pair of names collated together, the names in each pair already passed were separated by that boundary, then the names are said to be passed in the *same sense*, and the collation is called a *positive* one. If, on the contrary, they are not so separated, the names are passing in *opposite senses*, and the collation is a *negative* one.

This definition applies primarily to uniform (closed) groups of the first order. But it may be applied also to terminated groups, provided we begin passing in review always from one of the terminal units.

Now, collations may themselves be regarded as units of thought; and, as will shortly be seen, ordered groups of them may be formed, which may be passed in review. In doing this it will be found that the whole group of collations is separated into two parts, containing positive and negative collations respectively, by a boundary formed by two limiting cases of collation, the nature of which I proceed to indicate.

I have already laid it down that in passing in review we pass in the same sense till the whole group has been reviewed; the unit under review is, so to speak, never to turn back. But this need not prevent the unit from stopping still for a while in one group, while it goes on in

the collated group; or to put it another way, we may in special cases change the old name of one unit for more than one new name.

(2) Such a collation I will call a discontinuous one, and it may be shown that: *In a discontinuous collation, an unit which is collated with more than one unit must be collated with a continuous group of units, no two of which are separated by any two units with which the former unit is not collated.*

(3) Moreover, it may be shown that in discontinuous collations in the catalogue: *If any name within a group of units collated discontinuously with a given name* (not a terminal name of the group) *is also collated discontinuously, then the groups of names collated with single names must in both bases consist of the whole catalogue of names.*

As a matter of fact, this is the only sort of discontinuous collation with which we shall have to deal, and we may, therefore, say that besides positive and negative collations there are *zero* collations in the catalogue, in which one old and one new name (which may or may not be the same name) is collated in each case with all the names in the catalogue.

As an illustration, consider the specimen scheme given above. If we pass in review the names in the upper line as they are written, the initial collation is (A_1B_2), and we shall not reach any given collation before the old name reaches B_1. Let us, therefore, try the other way round, passing from A_1, to Z_1, X_1, P_1, K_1, and so on. The first given collation which we reach is (KL). Now, on the upper line, we have been passing names in the same part with respect to the boundary (AB) as K is, but on the lower line all we can say is that the units we have been passing have been in one of the two parts into which (BL) divides the catalogue, but we cannot say which until we know what catalogue name is represented by x; for x is not a catalogue name. We cannot yet say whether the old and new names already passed are or are not separated by (AB), or whether the collation is positive or negative.

(4) *We cannot determine the sense of a collation, unless the collations of at least three pairs of units are known.* But given this much, the sense of a collation is determined: for I may say at once that we shall not have anything to do with cases like that represented in the specimen scheme above, where part of the collation is continuous and part discontinuous. In all schemes in which the indeterminate symbol appears, zero collations are represented; and the rest of the line containing the symbol will therefore consist of repetitions of a single letter.

Let us now consider any four units in a group of the first order; say the names C, F, L, and Q; and let us pass the catalogue in review, starting from C.

Suppose the first of the named units which we come to is Q. Then (CQ) forms a boundary, and with respect to it L and F are in the same part of the group, which is passed after Q.

But we might equally well have passed this part of the group first. And in that case either F or L must have been the first named unit to be reached. Our knowledge of the alphabet tells us that it would have been F.

Thus in neither case could we reach L without passing either Q or F first. That is, (FQ) is a boundary, which separates C and L. Similarly it may be shown that (CL) forms a boundary separating F and Q.

(5) It follows that: *Of the three pairs of pairs which can be formed of any four units in a group of the first order, one consists of two pairs which mutually separate one another, while the other two consist each of two pairs which do not separate one another.*

Let us next discuss a collation in the catalogue, and pass in review, starting with (A_1K_2) as initial collation. We may denote the units

under review by x and y. And first, let the collation be a negative one, so that initially x and y are not separated by (AK). Nor does (Ax) initially separate (Ky). Therefore, initially, (Ay) must separate (Kx).

But, before x_1 reaches K_1, or y_2 reaches A_2 (whichever event occurs first), (Ay) has ceased to separate (Kx).

And, since neither has x been A_1, nor y K_2; x must have crossed the boundary (Ay) at y; and y crossed the boundary (Kx) at x.

That is to say, x and y must have stood for the same name, say C, which was therefore collated with itself.

Similar reasoning shows that after both x and y have passed K and A, they must come again to represent the same letter, say P this time, before they return respectively to A and K.

But after leaving C, x and y are separated by (CP) until they reach P together, and then are again separated by the same boundary until they reach C together.

(6) Therefore: *In any negative collation, two, and only two, units are collated each with itself; or, in the catalogue, two, and only two, names are left unchanged by a negative collation.*

If, however, the collation is a positive one, x and y are initially separated by (AK), and remain so until one or other of them crosses that boundary. The collation being a continuous one, if they reach K_1 and A_2 at the same time, they will cross the boundary together, and so still be separated by (AK) after they have crossed it, till they reach A and K once more, at the same time.

But if x does not reach K at the same time as y reaches A, then after one of these events, and before the other, we may have (Ax) separating (Ky), and (AK) no longer separating (xy). While this state of affairs lasts, therefore, x and y may come to mean the same name, once or more than once.

(7) But: *If (Ay) ever separates (Kx), at least two names must remain unchanged by the collation.* For y must have passed the boundary (Kx) at x, and will have to pass it back again before x and y have both passed the boundary (KA).

(8) Thus: *In a positive collation, there may be no name unchanged, or there may be any number of such names.*

In the case of zero collations it is obvious that one, or two, units collated discontinuously will each be collated with itself, amongst the rest, and that no other units can be so collated.

In general, if A_1 is collated with, say, K_2, there is no reason why K_1 should be collated with A_2; but such *reciprocal* collations are of peculiar significance in the theory of Order. We have, indeed, come across one example of this already. We found that if in a positive collation the units under review reached the initially collated units K_1 and A_2 at the same time, *i.e.* if A and K were reciprocally collated, they would, afterwards as before, be separated by (AK).

(9) That is: *If in a positive collation any one pair of units is collated reciprocally, then no unit is collated with itself.* It should, however, be noted that a single unit, collated with itself, is reciprocally collated; and this may occur once or oftener in a positive collation.

If in any collation every pair of units is collated reciprocally, the collation as a whole may be called reciprocal. In the case of a negative reciprocal collation, two units will be each reciprocally collated with itself. I use the terms *conjugate pairs* and *self-conjugate units* to denote such reciprocal collations. In the case of zero collations, if, say, A_1 is collated discontinuously, and any other letter, say K_2, is collated with it reciprocally, then K_1 is collated with A_2. But in a zero collation there is only one name in the second group collated with every name

in the first; this one name must, therefore, be A; and it follows, therefore, that the same name is collated discontinuously in both groups.

(10) Thus we conclude: *In a positive reciprocal collation every conjugate pair separates every other; in a negative reciprocal collation no conjugate pair separates any other; in a zero reciprocal collation, all conjugate pairs have a common unit.*

(11) And: *In a positive reciprocal collation no unit is self-conjugate; in a negative reciprocal collation two units are self-conjugate, and they form a self-conjugate boundary, which separates the two units of every self-conjugate pair; in a zero reciprocal collation it is the same unit in both groups which is collated discontinuously in both groups, and it alone is self-conjugate.*

E. T. Dixon.

(*To be continued.*)

PROOF BY RECURRENCE.

In its most simple and general form the proof by recurrence of a formula containing constants and one variable consists in giving a number of successive integral values to the variable, and showing by trial that the formula is true for those values. The next step is to show that the *form* of the result remains the same when $n+1$ is substituted for n, or that each result depends upon the preceding one. If this second step is neglected, as it often used to be, the proof seems to be invalid.

This proof by mathematical or successive induction has been very generally accepted by mathematicians and neglected by logicians.

Todhunter's[1] account of the process and application to the binomial theorem may be accepted as authoritative, from his large acquaintance with previous writers and his special study of the theory of probability: "We prove that if a theorem is true in one case, whatever that case may be, it is true in another case which we may call the *next* case; we prove by trial that the theorem *is* true in a certain case; hence it is true in the next case, and hence in the next to that, and so on; hence it must be true in every case after that with which we began." "Hence if the laws hold when $n-1$ factors are multiplied together, they hold when n factors are multiplied together; but they have been proved to hold when four factors are multiplied together, therefore they hold when five factors are multiplied together, and so on; thus they hold universally."

The article "Induction" (Mathematics) in the *Penny Cyclopædia* by de Morgan has been so long out of print that a mere reference would be of little use: "The method of induction, in the sense in which the word is used in natural philosophy, is not known in pure mathematics. There certainly are instances in which a general proposition is proved by a collection of the demonstrations of different cases, which may remind the investigator of the inductive process, or the collection of the general from the particular. Such instances, however, must not be taken as permanent, for it usually happens that a general demonstration is discovered as soon as attention is turned to the subject.

"There is, however, one particular method of proceeding which is extremely common in mathematical reasoning, and to which we propose to give the name of *successive induction*. It has the main character of induction in physics, because it is really the collection of a general truth from a demonstration which implies the examination of every particular case; but it differs from the process of physics, inasmuch as each case depends upon the one which precedes. Substituting, however, demonstration for observation, the mathematical process bears an analogy to the experimental one,

[1] *Algebra*, 272, 288.

which in our opinion is a sufficient justification of the term 'successive induction.'" "There are cases in which the successive induction only brings any term within the general rule when two, three, or more of the terms immediately preceding are brought within it. Thus, in the application of this method to the deduction of the well-known consequence of $x+\frac{1}{x}=2\cos\theta$, namely $x^n+\frac{1}{x^n}=2\cos n\theta$, it can only be shown that any one case of this theorem is true, by showing that the preceding two cases are true."

The most recent exposition of proof by recurrence is given by Prof. H. Poincaré.[1] It is stated to be the mathematical reasoning *par excellence*, and the general proof of the ordinary rules of arithmetic; further, it is claimed that mathematical induction is an affirmation of a property of the mind itself, and therefore far more certain than induction from observations or experiments which is based upon the belief in the general order of the universe.

Mill,[2] without discussing the validity of proof by recurrence, remarks: "Such cases as these are but examples of what I have called induction by parity of reasoning, that is not really induction because not involving inference of a general proposition from particular instances."

Stanley Jevons,[3] who differs so much from Mill on the general theory of induction, lays down very definitely: "That no finite number of instances can ever prove a general law, or can give us certain knowledge of even one other instance."

Babbage[4] gives a curious and powerful argument in the same sense from results obtained or obtainable by his calculating machines. He states that a machine can be made which will give a hundred millions and one of the series of natural numbers, but in the following 2761 terms ten thousand times the successive triangular numbers will be automatically added, after which the next 1430 terms follow another law; and then there appears a third new law for 950 terms more. Hence no series of recurrences, however long, can foretell the advent of the term or terms which apparently belong to a different law, but have been provided for in the original design of the machine.

Some of those, who have freely used proof by recurrence, seem to have had lingering doubts as to its validity, even in the case of integral values of the variable, since many mathematicians, such as James Bernoulli, Landen, Epinus, Euler, L'Huilier, Stewart and Lagrange, have given other proofs of the binomial theorem. There seems considerable justification for such doubts, especially when reliance is placed merely upon a series of recurrences proved by trial, and not upon the form of the result in successive instances.

The following formulae give series of 16, 29 and 40 primes:

$$x^2+x+17\,;\quad 2x^2+29\,;\quad x^2+x+41\,;$$

but, of course, all such formulae must fail when the value given to the variable is equal or bears a certain relation to the constant; hence no formula is known which will give a prime for every integral value of the variable. But in the above cases the proof by recurrence might be considered to be ample.

The difference between valid and invalid cases of proof by recurrence is often felt intuitively, but no general method of discriminating seems to be known, nor is it clearly shown why, if it be valid for positive integral values of the variable, it should be invalid in some cases for fractional or negative values.

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[1] *Science and Hypothesis*, 9 *et seq.* [2] *Logic*, i. 325.
[3] *Principles of Science*, 231. [4] *Ninth Bridgewater Treatise*, 34.

A formula may be true and intelligible for all values of the variable except one, thus $\int x^n dx$ is $x^{n+1}/n+1$ except in the case that $n=-1$ when it is $\log x$; or it may be only true and intelligible for one value of the variable; $y-b=\sqrt{-1}\,(x-a)$ is the equation to a straight line, and is satisfied if $x=a$ and $y=b$, which represents a point in the plane of reference, but every other point of the line is in the plane perpendicular to the plane of the paper.

More generally a formula is true and intelligible for certain values of the variable, but untrue or unintelligible for intermediate values. When these singular values are expressed graphically, they appear as conjugate points or pointed branches.

For positive values of x the equation $y=ax^2+\sqrt{x}\sin bx$ gives a continuous curve looped about a parabola, which it cuts when bx is a multiple of π. But when x is negative y is impossible except when $\sin bx=0$, or the figure consists of an infinite number of conjugate points situated on the negative branch of the parabola with abscissae π/b, $2\pi/b$, $3\pi/b$, etc.

Such cases suggest reasons for excluding negative but not fractional values of the variable.

Proof by recurrence may also be regarded as a special case of the probability of causes, or of the probability of future events derived from experience. The question to be decided is, a given formula gives true results in certain cases, what is the chance that the formula is exact and will give true results in every case, or that it is inexact and will fail in certain cases?

Problems of this kind are generally considered to be identical with the determination by a succession of drawings of the relative numbers of white and black balls in an urn, the total number of which may be known, unknown, or infinite.

The chance of drawing a white ball evidently depends upon the relative number of white and black balls in the urn. If the number of each kind be very large compared to the number of drawings, or if each drawn ball be replaced, this relation remains constant; but it more nearly corresponds to the case of mathematical or physical formulae, if the drawn balls are not replaced and the relation is treated as variable.

Before a trial is made, we may make any *assumption* as to the relative number of white and black balls in the urn. The most obvious supposition is that every possible combination is equally possible, or that the probability of each is $\frac{1}{n+1}$. Or we may suppose that the white and black balls have been selected by tossing a coin, when the probability of each combination is expressed by a term of the binomial expansion $(\frac{1}{2}+\frac{1}{2})^n$ or the relative probability is $1, \frac{n}{1}, \frac{n.\overline{n-1}}{1.2}, \frac{n.\overline{n-1}.\overline{n-2}}{1.2.3}$, etc.

It seems likely that the greater the number of trials, the greater is the chance of estimating the relative numbers of white and black balls remaining in the urn. But to take an extreme case, suppose that $n-1$ white balls have been drawn and not replaced, leaving only one in the urn, what is the probability that the last ball is white?

There are only two possible cases: the urn may have originally contained n white balls, or it may have contained $n-1$ white balls and one black. In the first case the remaining ball is certainly white, and in the second certainly black. On the *assumption* that each case is, à priori, equally probable, the chance that the last ball is white is $\frac{1}{2}$. On the second à priori *assumption* the possible hypotheses give for the probability of the event in the first case 1, in the second case $\frac{\lfloor n-1}{\lfloor n}$ or $\frac{1}{n}$, and in the other cases 0;

hence the probability that the fifth ball is white is

$$\frac{1}{1+n\,.\,\frac{1}{n}+0}=\tfrac{1}{2}, \text{ as before.}$$

Suppose, as before, that the unknown proportion of white balls, which is identical with the probability x of drawing a white ball, represents the chance that a formula is valid or universally true, and that the chance of drawing a black ball $(1-x)$ is the chance that the formula is not universally true, or that the favourable instances have occurred by accident, and to obviate arithmetical difficulties let each drawn ball be replaced and the urn shaken.

The accepted treatment of the problem is somewhat as follows: since the relative proportion of white and black balls is unknown, assume that all values of x are equally probable. If $m+n$ drawings give m white and n black balls, the probability which any assumed value of x gives for the observed event is $x^m(1-x)^n$. On differentiating this expression, the maximum value for x is found to be $m/m+n$.

Bayes assumed that x may vary infinitesimally between the limits 1 when all the balls are white, and 0 when all the balls are black. But if a white occurs more frequently than a black ball, it is a fair assumption that the probability of a cause producing a white ball is $>\frac{1}{2}$, and increases with the number of occurrences. Hence the probability of a cause which produces a white ball is

$$P=\frac{\int_{\frac{1}{2}}^{1} x^m(1-x)^n dx}{\int_0^1 x^m(1-x)^n dx}.$$

But a black ball at once shows that the formula is invalid, and if $n=0$ the probability is $P=\int_{\frac{1}{2}}^{1} x^m dx \Big/ \int_0^1 x^m dx$ or $1-\left(\frac{1}{2}\right)^{m+1}$.

Comparatively few trials give high values for P, if $m=5$, $P=63/64$. But it must be clearly remembered that this only means that there is a high probability, that the chance of the existence of a cause lies between $\frac{1}{2}$ and 1, or that the formula is more likely to be valid than invalid. The same formula may be used to calculate the probability that the existence of a cause is greater than any assigned limit. After 100 white balls have been drawn, to find the probability that the urn contains at least 9999 white to one black ball. $P=1-(\frac{9999}{10000})^{101}=1/100$ nearly.

The probability of a considerable chance that the formula is universally true is small, and to increase it to $\frac{1}{2}$ about 6930 trials would be required. According to Bayes' theorem, then, no finite number of recurrences can make a formula more than highly probable, and even to get so far, the successful trials must be very numerous.

Assuming the same data, Laplace obtained a different solution. The probability that x lies between x and $(x+dx)$ is expressed by $cx^m(1-x)^n dx$, where c is a constant independent of x. But the probability that x lies between 0 and 1 is certain.

$$\therefore\ 1=c\int_0^1 x^m(1-x)^n dx=cB\,.\,(m+1,\ n+1)=c\,\frac{\Gamma(m+1)(n+1)}{\Gamma(m+n+2)};$$

$$\therefore\ c=\lfloor m+n+1 \,/\, \lfloor m \,\lfloor n.$$

Hence the probability of the hypothesis, that the ratio between the white and black balls is $x/(1-x)$, is $\dfrac{\lfloor m+n+1}{\lfloor m\,\lfloor n}\,x^m(1-x)^n dx$. And on the assumption that this ratio is correct if $(\mu+\nu)$ future drawings be made, the most probable

case is that the ratio μ/ν is the same as the ratio m/n, and the probability of this occurrence is expressed by $\frac{\underline{|\mu+\nu}}{\underline{|\mu}\,\underline{|\nu}}x^{\mu}(1-x)^{\nu}dx$. But the probability of the compound event is the product of the two separate probabilities, or

$$P=\frac{\underline{|\mu+\nu}}{\underline{|\mu}\,\underline{|\nu}}\;\frac{\underline{|m+n+1}}{\underline{|m}\,\underline{|n}}\int_0^1 x^{m+\mu}(1-x)^{n+\nu}dx$$

$$=\frac{\underline{|\mu+\nu}}{\underline{|\mu}\,\underline{|\nu}}\;\frac{\underline{|m+n+1}}{\underline{|m}\,\underline{|n}}\;\frac{\underline{|m+\mu}\,\underline{|n+\nu}}{\underline{|m+\mu+n+\nu+1}}.$$

This formula has been frequently applied by Laplace and others generally to find the chance of one future occurrence after a series of favourable trials, in which case $n=0$, $\mu=1$, $\nu=0$, and the factorials reduce to $P=\frac{m+1}{m+2}$.

Thus Laplace finds a very high probability that the sun will rise to-morrow. After 6930 white balls have been drawn, the chance that the next one will be black is only 1/6932, which may be compared with the somewhat indefinite result obtained by Bayes' theorem.

Bertrand,[1] after giving the proof of the formula, significantly remarks: "The applications which have been made of this formula are almost all without foundation." He derides the exactness of the result obtained in the case of the sun.

Boole[2] dissents from the validity of both these solutions: "When the defect of data is supplied by hypothesis, the solutions will in general vary with the nature of the hypothesis assumed; so that the question still remains only more definite in form, whether the principles of the theory of probabilities serve to guide us in the election of such hypothesis. I have already expressed my conviction that they do not."

If the validity of Laplace's formula be admitted, no finite number of favourable instances can make the validity of a general formula certain, and a very long series of recurrences is required to render comparatively few future terms even somewhat highly probable.

Whether mathematical induction or proof by recurrence is to be regarded as a true induction or not, depends upon the exact definition given of the latter process.

Logicians differ much as to this definition, and some have even given two or more definitions not exactly in accordance with one another.

Whately[3] defines induction as "A kind of argument which infers respecting a whole class what has been ascertained respecting one or more individuals of that class."

Mill[4] states, "Induction may be defined, the operation of discovering and proving general propositions"; where 'prove' must be taken in its original sense of testing by trial or rendering probable. And again, "Induction is that operation of the mind, by which we infer that, what we know to be true in a particular case or cases, will be true in all cases which resemble the former in certain assignable respects."

It seems as though proof by recurrence falls within any of these definitions, but Mill specially classes it among inductions improperly so-called, and instances Newton's proof of the binomial theorem.

The steps by which a valid induction is reached appear to be four in number:

We collect a considerable number of similar instances, or, in the words of Whewell, we form a colligation of facts.

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[1] *Calcul des Probabilités*, 172. [2] *Laws of Thought*, xx.
[3] *Logic*, 267. [4] *Logic*, III. i. and ii.

We abstract in each case the circumstances common to all and material to our argument from others which are not common to all or immaterial to our argument.

We then connect the common circumstances by a rule or formula which contains them all. In attempting to find a rule or formula it is often difficult to distinguish and isolate the true argument which connects the instances. When this has been effected, the form of the formula may often be obtained from theoretical considerations, or from inspection of the graphical representation of the instances, or finally, in case of failure, recourse must be had to the more or less blind trial of hypotheses.

Lastly, the rule or formula must be verified by showing that it applies to other facts either similar to or different from those already ascertained.

The colligation of facts is a necessary preliminary to every induction, since a single instance is a very insecure foundation, but it is not itself an induction. The idea of induction contains as a necessary element a leap into the unknown, or an assumption that cases which have not been considered will fall under the same rule as those which have been already investigated. This leap of the mind into the unknown introduces an element of doubt into every induction, which is more or less removed by subsequent verification. If the verification is perfect by the examination of every possible instance, the doubt ceases, but the rule also ceases to be an induction and becomes a mere colligation of facts.

The above view of induction may be applied to Newton's[1] statement of his discovery of the binomial theorem. "I applied myself, therefore, to discover a method by which the first two terms of this series might be derived from the rest; and I found that if for the second figure or numerical term I put m, the rest of the terms would be produced by the continual multiplication of the terms of the series: $\frac{m-0}{1} \times \frac{m-1}{2} \times \frac{m-2}{3} \times \frac{m-3}{4}$, etc. For instance, if the second term be put for 4, there will arise $4 \times \frac{m-1}{2}$, that is 6, which is the third term; the fourth term will be $6 \times \frac{m-2}{3}$, that is 4; the fifth term will be $4 \times \frac{m-3}{4}$, that is 1; and the sixth term will be $4 \times \frac{m-4}{5}$, that is 0, which shows that the series is here terminated in this case.

"This being found, I applied it as a rule to interpolate the above-mentioned series."

It seems then that Newton was led to the form of the coefficients by the mere consideration of successive terms in one or more expansions. From the consideration of these instances his mind leapt to the dependence of each term on the one before it, and the rule connecting them. His intuitive genius caused him to be satisfied with a comparatively small amount of verification, and extended the theorem in the same manner to negative and fractional indices. Universal experience has since shown his assumption to be correct, and various general proofs have been given.

If a mathematical formula is true, it is generally assumed to be true universally and exactly, except in those comparatively rare instances in which small terms have been omitted or approximate values introduced for some definite purpose. Hence a mathematician has no compunction in applying a formula to cases intermediate, between, or far beyond those which have been rendered certain by trial.

Experimental values, on the other hand, are not exactly true, and can only be more or less approximately represented by a formula. Hence,

[1] Horsley, iv. 540.

unless his experimental values are fairly close together, a physicist is chary in applying his formulae to intermediate values, or in any case much beyond the extreme values proved by his experiments.

In several well-known cases, *e.g.* Fermat, too much reliance seems to have been placed on proof by recurrence; and the consideration of a simple case may be suggestive.

The formula n^2+n+41 gives a prime for all values of n from 0 to 39; the following 81 values give primes in all except 18 cases, and in those cases the factors are two primes. Trusting entirely to proof by recurrence, the first thirty-nine values of n would seem to show that the formula always gives a prime.

Excluding 0, the chance that a value for $n<121$ gives a prime is $\frac{102}{120}$, and if the first trial gives a prime, the chance of a prime at the second trial is $\frac{101}{119}$, and so on. Hence, writing x for one less than the number of trials, the chance of prime for $x+1$ trials is

$$\frac{102}{120}\cdot\frac{101}{119}\cdot\frac{100}{118}\cdots\frac{102-x}{120-x} \text{ or } \frac{\lfloor\underline{102}}{\lfloor\underline{120}}\times\frac{\lfloor\underline{120-x}}{\lfloor\underline{102-x}}.$$

And the chance of not-prime appearing in $x+1$ trials is

$$1-\frac{\lfloor\underline{102}}{\lfloor\underline{120}}\times\frac{\lfloor\underline{120-x}}{\lfloor\underline{102-x}}.$$

When any fractional value is given to this expression, the value of x can be found by trial.

Thus, how many trials must be made that the chance of falling upon not-prime is $\frac{9999}{10000}$?

The value of x is found to be about 44, and the chance that forty-five trials give white is $\frac{1}{11360}$.

If $x=102$, $\frac{\lfloor\underline{102}}{\lfloor\underline{120}}\times\frac{\lfloor\underline{18}}{\lfloor\underline{0}}=1/1.0867\times10^{21}$, or the chance that a negative instance is obtained is very great. If 104 or more trials are made, a negative instance must be obtained, or the value of the factorial expression should be 0. Since $\lfloor\underline{-1}=\Gamma 0=\infty$, which is also true for all negative integral values of the gamma function, this is seen to be the case. SYDNEY LUPTON.

APPRECIATIVE REMARKS ON THE THEORY OF GROUPS.[1]

WHILE it is clearly impossible for the average high school teacher of mathematics to become familiar with all the modern branches of this subject, it is desirable that he should not be totally ignorant of any extensive branch. The views of a number of eminent mathematicians often furnish one of the simplest as well as one of the most reliable introductions to the nature and the scope of a difficult subject. The following list of quotations has been prepared for the purpose of providing such an introduction for one subject. The list could easily be extended, but the variety and the standing of the mathematicians quoted are perhaps sufficient to inspire confidence. The

[1] Reprinted by permission from *School Science and Mathematics*, with some alterations and additions by the author.

quotations are arranged chronologically, beginning with 1892. Several of them were published in the *American Mathematical Monthly*, volume x. 1903, p. 87.

"The theory of congruences bases itself substantially upon a fundamental concept of mathematics, which is already the foundation of Poinsot's method, the concept of group."[2] "The group concept was employed in the preceding century (about 1770) simultaneously by Lagrange and Vandemonde, and since this time it occupies a prominent place in the theory of algebraic equations. In regard to this it is only necessary to refer to the name of Galois. Hence group theory has been regarded as a supplement of algebra. This, however, is incorrect, for the group concept extends far beyond this into almost all parts of mathematics."[3]

"The mathematics of the twenty-first century may be very different from our own; perhaps the schoolboy will begin algebra with the theory of substitution groups, as he might now but for inherited habits."[4] "A large part of the theory of numbers is only the theory of abelian groups."[5] "The concepts of group and invariant take each day a more preponderant place in mathematics and tend to dominate this entire science."[6] "Although we cannot give here a complete exposition of the fundamental results of Sophus Lie in the theory of continuous transformation groups, yet it is indispensable that we make some general remarks on the notion of the continuous groups which play such an important *rôle* in the science of our epoch."[7]

"Galois showed that to each algebraic equation there corresponds a group of substitutions in which the essential characteristics of the equation are reflected. In algebra the theory of groups has been studied at an early date, especially by Cauchy, who introduced certain elements of classification. The study of Galois in the theory of equations exhibited the importance of the notion of invariant subgroup of a given group, and led him to divide the groups into simple and composite, a fundamental distinction which really extends far beyond the domain of algebra to the concept of groups of operations in the most comprehensive accepted meaning of this term."[8] "The whole subject of geometry may be regarded as a theory of continuous groups which define all possible collineations and motions."[9] "The theory of groups, which is making itself felt in nearly every part of higher mathematics, occupies the foremost place among the auxiliary theories which are employed in the most recent function theory."[10]

"There are two things which have become especially important for the latest developments of algebra; that is, on the one hand the ever more dominating theory of groups whose systematising and clarifying influence can be felt everywhere, and then the deep penetration of number theory."[11] "In fine, the principal foundation of Euclid's demonstrations is really the existence of the group and its properties. Unquestionably he appeals to other axioms which it is more difficult to refer to the notion of group. An axiom of this kind is that which some geometers employ when they define a straight line as the shortest distance between two points. But it is precisely such axioms that Euclid enunciates. The others, which are more directly associated with the idea of displacement and with the idea of groups are the

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[2] Bachmann, *Die Elemente der Zahlentheorie*, vol. 1, 1892, Preface.

[3] Klein, *Einleitung in die höhere Geometrie LL.*, 1893, p. 3.

[4] Newcomb, *Bulletin of the American Mathematical Society*, vol. 3, 1893, p. 107.

[5] Frobenius, *Berliner Sitzungsberichte*, 1893, p. 627.

[6] Lie, *Centenaire de l'École Normale*, 1895, p. 485.

[7] Picard, *Traité d'analyse*, vol. 3, 1896, p. 492.

[8] Picard, *Œuvres mathématiques de Galois*, 1897, Introduction.

[9] Russell, *Foundations of Geometry*, 1897, p. 47.

[10] Fricke und Klein, *Automorphe Functionen*, vol. 1, 1897, p. 1.

[11] Weber, *Lehrbuch der Algebra*, vol. 1, 1898, Preface.

very ones which he implicitly admits, and which he does not even deem necessary to enunciate. This is tantamount to saying that the former are the fruit of later experience, that the others were first assimilated by us and that consequently the notion of group existed prior to all others."[12]

"The most important of all these viewpoints is furnished by the theory of groups, which is really a creation of our century, and has shown its dominating influence in nearly all parts of mathematics; not only in the recent theories but also far towards the foundation of the subject, so that this theory can no longer be omitted in the elementary text-books."[13] "I should reproach myself for forgetting, even in so rapid a *résumé*, the applications which Lie has made of his theory of groups to the non-Euclidean geometry and to the profound study of the axioms which lie at the basis of our geometric knowledge."[14] "I desire to offer to the young students of the Italian universities a book which will introduce them to the study of one of the most important theories (groups) of modern mathematics."[15]

"It might be said of the most important parts of recent geometry that one conception dominates everywhere; that is, the conception of group."[16] "A department of mathematics that is universally acknowledged to be of fundamental importance is the theory of groups."[17] "The notion of transformation and of group acquires in the mathematical sciences, in analysis as well as in geometry, a more and more prominent place."[18] "In *résumé* we may thus say that the group concept, hardly noticeable at the beginning of the century, has at its close become one of the fundamental and most fruitful notions in the whole range of our science."[19]

"The important place which the theory of groups has taken in modern mathematics, principally through the works of Lie, is known. Its applications extend to higher arithmetic, algebra, analysis, geometry and mechanics."[20] "The comprehensive meaning of the group concept, much more extensive than its earliest applications, appeared first in the works of C. Jordan. Starting from these the works of F. Klein and S. Lie made this concept a special object of investigation and brought it into the centre of mathematical research."[21] "Sets, systems and groups, these three words are the technical names for conceptions which are to be met with in all branches of mathematics."[22] "The fundamental *rôle* which the notion of group plays in mathematics is known. It furnishes a guiding principle which cannot be too clearly exhibited and which ought to inspire the teachers more and more, but we do not yet possess an exposition which confines itself to the most elementary applications."[23]

"Among the words which have exercised the most desirable influence are those of group and invariant. They have enabled us to see the real essence of many mathematical thoughts, and have shown us that the ancient mathematicians employed groups in many cases without knowing it, and how they found themselves all at once together without knowing why, when they had considered themselves far apart."[24]

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[12] Poincaré, *The Monist*, vol. 9, 1898, p. 34.

[13] Pund, *Algebra mit Einschluss der elementaren Zahlentheorie*, 1899, Preface.

[14] Darboux, *Comptes Rendus*, vol. 128, 1899, 528.

[15] Bianchi, *Lezioni sulla teoria dei gruppi di sostituzioni*, 1900, Preface.

[16] Maschke, *American Mathematical Monthly*, vol. 9, 1902, p. 214.

[17] Major MacMahon, *Nature*, vol. 65, 1902, p. 448.

[18] Richard, *Sur la philosophie des mathématiques*, 1903, p. 229.

[19] Pierpont, *Bulletin of the American Mathematical Society*, vol. 11, 1904, p. 144.

[20] Couturat, *Les principes des mathématiques*, 1905, p. 329.

[21] Fano, *Encyklopaedie der Mathematischen Wissenschaften*, vol. 3, 1908, p. 293.

[22] Bôcher, *Introduction to Higher Algebra*, 1907, p. 80.

[23] Fehr, *L'Enseignement Mathématique*, vol. 9, 1907, p. 192.

[24] Poincaré, *Bulletin des Sciences mathématiques*, vol. 32, 1908, p. 175.

"The new foundation (of elementary geometry) has been laid in the nineteenth century by the works of leading mathematicians. It consists of the recognition that elementary geometry is equivalent to the investigation of the group of movements. Such a view is in accord with the characteristic tendency of modern natural scientists to replace static investigations of the phenomena by dynamic; or, to speak in more general terms, the thought of development penetrates more and more our observations."[25] "Galois directed attention to the use of discontinuous groups in algebra. At a later date this concept was employed in number theory by Dedekind and others, and in the theory of functions by Klein, Poincaré, Picard and others. The theory of the solution of algebraic equations is principally based on the consideration of certain groups of substitutions or permutations of the roots."[26]

To form a just estimate of the value of the preceding quotations it is necessary to bear in mind that they are, to a large extent, taken from the writings of very eminent mathematicians, including Poincaré, Picard, Darboux, Klein, Frobenius, and Weber. It should also be observed that they relate to a number of the large domains of pure mathematics, with explicit mention of number theory, theory of equations, theory of functions, and both elementary and higher geometry.

In a general way it may be said that the theory of groups forms the framework or skeleton of many mathematical theories. It does not lack symmetry or suggestiveness in itself, but to many it is not attractive in its bare form. With the proper trimmings and enlargements its usefulness and attractiveness become pronounced, and they have led to numerous such enthusiastic statements as are illustrated by some of the quotations given above.

In his recent Presidential Address published in the *Proceedings of the London Mathematical Society*, vol. vii. 1909, Professor Burnside calls attention to the fact that the theory of groups of finite order has not received due attention on the part of English mathematicians, and he incidentally points out an easy road into some of the elements of this theory. The general abstract theory of groups is commonly divided into four parts. Two of these relate to discrete groups of finite and of infinite orders respectively, while the other two relate to continuous groups of finite and of infinite orders. All of these are closely related, and owe their origin and many of their guiding principles to the first one.

G. A. MILLER.

University of Illinois, Urbana.

MATHEMATICAL NOTES.

344. [**R. 4. a.; V. a.**] Exception is taken, p. 163, Vol. V., to an examination question in Statics about two ladders, where it comes about that their inclinations are determined by the equations

$$9\tan\theta = 16\tan\phi, \quad \text{.................................(1)}$$

$$16\cos\theta + 9\cos\phi = 16{\cdot}8. \quad \text{................................(2)}$$

The candidate is asked to prove that "*the two ladders will be at right angles to one another when in equilibrium*," and it is assumed that for this he has to solve the equations (1), (2) *ab initio*. Yet surely all that is required is to show that $\phi = \frac{\pi}{2} - \theta$ *does* give a solution, and that there can be *no other*. And this is readily done; for, by (1), we have $\tan\theta = \cot\phi = \frac{4}{3}$ (taking the positive root), and then $\cos\theta = \frac{3}{5}$, as $\cos\phi = \frac{4}{5}$, and (2) is also satisfied. More

[25] Borel, *Die Elemente der Mathematik*, vol. 2, 1909, Preface.

[26] Mueller, *Führer dürch die mathematische Literatur*, 1909, p. 64.

over, there can be no other solution; for if θ is less, ϕ is also less by (1), and therefore $16\cos\theta+9\cos\phi$ is increased beyond its proper value. This is merely intended to show that such a problem need not make very serious demands upon a candidate. I have not seen the paper in question, so do not profess to judge whether it was a fair one on the whole.

PERCY J. HEAWOOD.

Several correspondents have suggested that the criticism of this problem was perhaps too severe. [Ed.]

345. [V. a.] Among the criticisms on Euclid's methods, one turns up, not infrequently, which seems singularly unfair. It is that the axiom that "two straight lines cannot enclose a space" is involved in certain propositions where it is not explicitly referred to, and it is inferred that the logic is at fault. [See *e.g. Gazette*, vol. v. p. 88, Note 289.] Surely it is a commonplace that this axiom forms practically Euclid's *definition* of straightness, and is involved in his whole treatment of the geometry of the straight line, though explicitly referred to where it is first pointedly involved--in his first theorem. Of course we may object to this interrelation of definition and axiom, but then the criticism should be directed to this technical point, and not to the validity of individual proofs based on this understanding.

PERCY J. HEAWOOD.

346. [V. a.] *Squared paper solution of the equation* $a\cos\theta+b\sin\theta=c$.

The problem may be stated thus—Rectangular axes are turned through the angle θ; the new coordinates of a certain point are (b, a), and its old ordinate was c: find θ.

Mark in a figure the point $P(b, a)$ and draw the line $y=c$. Let Q, Q' be the points where this line cuts the circle through P with the origin as centre. Then the two values of θ are POQ, POQ'.

A. C. DIXON.

347. [I. 2. b.] I have just read in the May number of the *Gazette* the interesting article by Sydney Lupton on "Furor Arithmeticus." On page 276 he says that "tables of prime factors up to 9,000,000 have been printed . . . , and that for the tenth million is said to exist in MS. at Berlin." It should have been noted that in 1909 the Carnegie Institution of Washington, D.C., U.S.A., published the table by D. N. Lehmer entitled, "Factor table for the first ten millions, containing the smallest factor of every number not divisible by 2, 3, 5, or 7 between the limits 0 and 10,017,000."

In the introduction to this table, Dr. Lehmer gives a short historical statement as regards other factor tables, and refers to a MS. of Kulik which was placed in the charge of the Vienna Royal Academy in 1867. "These tables are said to give the smallest factor of all numbers not divisible by 2, 3, or 5 up to the limit of *one hundred million*!" Lehmer's work includes a number of corrections of errors found in other tables.

G. A. MILLER.

348. [K. 7. d; M'. e. 8.] *On Note* 339, p. 386, vol. v.

If O_1, O_2 are conjugate with respect to each conic of the system through P, Q, R, S, to prove that $O_1(PQRS)=O_2(O_1ABC)$, where A, B, C are the intersections (PQ, RS), (PR, QS), (PS, QR).

Let U_1, U_2 be the conics O_1PQRS, O_2PQRS; then U_1, U_2 both touch O_1, O_2. Let L_1, L_2, M_1, M_2, N_1, N_2 be the other common tangents of U_1, U_2. Then it is known that O_2L_2, O_2M_2, O_2N_2 pass through A, B, C respectively, and also that the cross-ratio in the one conic of the four common tangents is equal to that of the four common points in the other, that is,

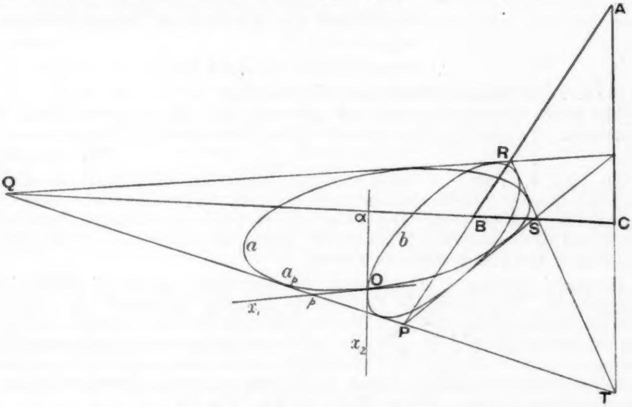
$$O_1(PQRS)=O_2(O_2L_2M_2N_2)$$
$$=O_2(O_1ABC).$$

The order of rays in the second pencil is determined by the consideration that P, Q, R, S are the poles of O_1O_2, L_1L_2, M_1M_2, N_1N_2 as to a conic V, one of the four with respect to which U_1, U_2 are polar reciprocals.

A. C. Dixon.

Let PQRS be a quadrilateral, ABC its diagonal triangle, and let Ox_1, Ox_2 *be two lines conjugate for the range of conics inscribed in the quadrilateral. Let one of them,* Ox_1, *meet the lines PQ, QR, RS, SP in the points p, q, r, s; and let the other,* Ox_2, *meet the lines BC, CA, AB, in the points* α, β, γ.

Then will the range $(pqrs) =$ *the range* $(O\alpha\beta\gamma)$. (Correlative of Dr. W. P. Milne's theorem, *Math. Gaz.*, Jan. 1911, p. 386.)



Of the range of conics, if we draw the two a, b, touching respectively the conjugate lines Ox_1, Ox_2, they will have O for a common point. Let a touch the lines PQ, QR, RS, SP in the points a_p, a_q, a_r, a_s.

Then the polar of P for a passes through C. Let this polar meet Ox_1 in y. Then, since P is the pole of Ca_p, and O is the pole of Oy, therefore y is the pole of PO.

Therefore Py, PO are conjugate lines for a. ....(1)

$\therefore$ the range $(pqrs)$ on the tangent Ox_1

$=$ conic-pencil of points of contact (a_p, a_q, a_r, a_s)

$=$ range on the tangent PQ

$= (a_pQTP)$.

Now the eleven-tangent conic, which is the envelope of the polars of O for the range of conics, touches the lines BC, CA, AB, Ox_1, Ox_2, and also touches the line Py, since $P(yOa_pa_s)$ is harmonic by (1).

Let Py meet BC in k, and CA in l.

Then the range made by the four tangents Ox_1, BC, CA, AB on the two tangents Ox_2 and Py are equicross,

$$\text{i.e. } (O\alpha\beta\gamma) = (yklP) = C(yklP) = (a_pQTP) = (pqrs).$$

John J. Milne.

349. [K. 2. d.] *The Polar Circle.*

1. In the $\triangle ABC$, orthocentre H, draw AA_1 $\perp^r$ to BC. Then, since $HA \,.\, HA_1 =$ rad.^{2} of Polar Circle (P.C.), it follows that this circle belongs to the coaxal system, whose limiting points are A and A_1, and whose R.A. is $B'C'$, bisecting AB, AC.

Hence, if P be any point on the P.C.,

$$PA^2=2 . AH . PM;$$

and if Q be any point whatever,

$$QA^2-\text{Power of } Q \text{ for P.C.}=2 . AH . QN;$$

$$PM, QC \text{ being } \perp^{\text{rs}} \text{ on } B'C'.$$

2. I have shown elsewhere that, if Q be the centre of an in-conic, with foci S, S', and axes $2p$, $2q$,

$$\text{then } 4R . QN=SA . S'A.$$

$$\therefore\ 2 . AH . QN=SA . S'A . \cos A$$

$$=\tfrac{1}{2}(SA^2+S'A^2-4p^2), \text{ by a well-known theorem,}$$

$$=(QA^2-\overline{p^2+q^2}).$$

$$\therefore \text{ Power of } Q \text{ for P.C.}=p^2+q^2,$$

or the P.C. and Director Circle cut orthogonally.

[My proof (analytical) that $4R . QN=SA . S'A$ is somewhat long and cumbrous. I should be glad to meet with a short proof by Pure Geometry.]

W. GALLATLY.

350. [Vol. v. p. 396.] I think that Mr. Milne has made a slip in stating that Apollonius' "Inclinationes" was restored by Anderson in 1612. In *The Ladies' Diary*, 1840, Appendix, pp. 49-61, is given an "Abstract of the Principal Geometrical Writings of Alexander Anderson, of Aberdeen." The title of the first is given as follows :

Alexandri Andersoni, Aberdonensis Supplementum, Apollonii Redivivi (of Ghetaldus). *Sive Analysis Problematis hactenus desiderati ad Apollonii Pergaei doctrinam* περι νέυσεων, à Marino Ghetaldo Patritio *Ragusino hujusque, non ita pridem restitutam. In qua exhibetur mechanice aequalitatum tertii gradus sive solidarum, in quibus magnitudo omnino data, aequatur homogeneae sub altero tantum coëfficiente ignoto. Huic subnexa est variorum problematum practice eodem authore.* Paris, 1612. In 4to. Pp. 36.

In this book there is no treatment of the problem of inclinations.

R. C. ARCHIBALD.

Dr. Archibald is quite correct. I was misled by a reference in Hutton's *Mathematical Dictionary* (1796), vol. i., under the heading Apollonius, where he says, "Alex. Anderson, Supplem. Apol. Redivivi. Inclin. Paris, 1612, 4to." Not having the book, I was unable to observe the golden rule: "Verify your references."

J. J. MILNE.

[Barlow's *New Mathematical and Philosophical Dictionary* (1814) gives the identical reference, with the exception of "Red." for "Redivivi." W. J. G.]

351. [A. 1. a.] *Some Interesting Formulae in Arithmetical Progression.*

With the usual notation, $S_1=\frac{n}{2}(2a+\overline{n-1}b)$.

Let $M=\frac{a+l}{2}$, and let S_2, S_3 be the sums of the squares and cubes respectively of the terms of S_1.

Then $S_1=n(a-b)+\frac{n(n+1)}{2}b,$

$$S_2=n(a-b)^2+n(n+1)b(a-b)+\frac{n(n+1)(2n+1)}{6}b^2,$$

$$S_3=\left[n(a-b)+\frac{n(n+1)}{2}b\right]\left[(a-b)^2+(n+1)b(a-b)+\frac{n(n+1)}{2}b^2\right]$$

$$=S_1[bS_1+a(a-b)],$$

$$\text{and } =[bn^2+(2a-b)n][(bn^2+\overline{2a-b}n)b+2a(a-b)]/4.$$

$$S_1=nM\,;\quad S_2=n\left[M^2+\frac{b^2(n-1)}{\underline{|2}}\right];\quad S_3=nM\left[M^2+\frac{b^2(n^2-1)}{4}\right].$$

Hence the well-known relations :

$$n^2S_3-3nS_1S_2+S_1^3=0, \quad \text{.......................................}(1)$$

$$nS_2-S_1^2=b^2n^2(n^2-1)/12, \quad \text{.......................................}(2)$$

$$b=2\sqrt{3(nS_2-S_1^2)}/(n\sqrt{n^2-1}), \quad \text{..........................}(3)$$

$$a=S_1/n-b(n-1)/2. \quad \text{.......................................}(4)$$

From (1) we see that if the progression is in positive integers, we have $9S_2^2-8S_1S_3$ is always a perfect square.

These formulae are useful in the solution of otherwise not very easy problems :

(1) Find an A.P., given S_1, S_2, S_3.

Here (1) gives n ; (3) gives b when n is known ; and a follows by (4).

(2) Find an A.P., given S_1, S_2, n ; S_1, S_3, n ; or S_2, S_3, n. E. N. BARISIEN.

352. [V. 1. a.] We all owe Dr. Nunn a great debt for the fascinating article on Wallis and his marvellously ingenious evaluation of π. The thinking of $\sqrt{r^2-a^2}$ as $[\sqrt[\frac{1}{2}]{r}-\sqrt[\frac{1}{2}]{a}]^{\frac{1}{2}}$ is in itself very wonderful : and then to get the unknown σ by the artifice of imprisoning a long multiple of σ between two known sets of factors, and so getting limits as close as we like between which σ must lie, merely from consideration of a table, was an inspiration.

I notice that Wallis' table was formed apparently by clever guess work. But, as a matter of fact, it is easy to see by induction from any special case that the characteristic fraction for $[\sqrt[R]{r}-\sqrt[R]{a}]^P$ is $\dfrac{\underline{|R}\,\underline{|P}}{\underline{|R+P}}$; (I append the sort of work involved—quite elementary and simple, and rather pretty). Hence the figures in the table are $\dfrac{\underline{|R+P}}{\underline{|R}\,\underline{|P}}$. These are easily evaluated unless *both* R and P are fractional ; in that case we get σ and its multiples, σ being $\dfrac{\underline{|1}}{\underline{|\tfrac{1}{2}}\,\underline{|\tfrac{1}{2}}}$. So that the whole investigation is closely connected with the interesting extension of the factorial notation to fractions with $\underline{|\tfrac{1}{2}}=\tfrac{1}{2}\sqrt{\pi}$.

We see at once that the multipliers in each row are the successive values of $\dfrac{R+P}{P}$, or, to avoid complex fractions, $\dfrac{2R+2P}{2P}$.

Thus, if $R=\frac{1}{2}$, $P=\frac{1}{2}$ we get σ, and for $P=1\frac{1}{2}$, $2\frac{1}{2}$, $3\frac{1}{2}$, etc. we get $\sigma\times\frac{4}{3}\times\frac{6}{5}\times\frac{8}{7}\times\ldots$, and for $R=\frac{1}{2}$, $P=0$ we get 1, and for $P=1, 2, 3, \ldots$ we get $1\times\frac{3}{2}\times\frac{5}{4}\times\frac{7}{6}\times\ldots$. In fact, we get all Dr. Nunn's multipliers, *with the reasons* for them.

This investigation seems to explain why Euler's first integral should have been one with a solution of the factorial type like $\dfrac{\underline{|R+P}}{\underline{|R}\,\underline{|P}}$, and his second integral the Γ function itself, if he was led by Wallis' researches to hunt for a general method of evaluating fractional factorials.

$$\left(r^{\frac{1}{p}}-a^{\frac{1}{p}}\right)^4=r^{\frac{4}{p}}-4r^{\frac{3}{p}}.a^{\frac{1}{p}}+6r^{\frac{2}{p}}.a^{\frac{2}{p}}-4r^{\frac{1}{p}}.a^{\frac{3}{p}}+a^{\frac{4}{p}}.$$

$$\text{Characteristic fraction}=1-\frac{4}{1+\frac{1}{p}}+\frac{6}{1+\frac{2}{p}}-\frac{4}{1+\frac{3}{p}}+\frac{1}{1+\frac{4}{p}}$$

$$=p\left[\frac{1}{p}-\frac{4}{p+1}+\frac{6}{p+2}-\frac{4}{p+3}+\frac{1}{p+4}\right]$$

$$=p\left[\left(\frac{1}{p}-\frac{1}{p+1}\right)-3\left(\frac{1}{p+1}-\frac{1}{p+2}\right)+3\left(\frac{1}{p+2}-\frac{1}{p+3}\right)-\left(\frac{1}{p+3}-\frac{1}{p+4}\right)\right]$$

$$=p\left[\frac{1}{p(p+1)}-\frac{3}{(p+1)(p+2)}+\frac{3}{(p+2)(p+3)}-\frac{1}{(p+3)(p+4)}\right]$$

$$=p\left[\frac{1}{p(p+1)}-\frac{1}{(p+1)(p+2)}-2\left(\frac{1}{(p+1)(p+2)}-\frac{1}{(p+2)(p+3)}\right)\right.$$
$$\left.+\frac{1}{(p+2)(p+3)}-\frac{1}{(p+3)(p+4)}\right]$$

$$=1.2p\left[\frac{1}{p(p+1)(p+2)}-\frac{2}{(p+1)(p+2)(p+3)}+\frac{1}{(p+2)(p+3)(p+4)}\right]$$

$$=1.2p\left[\left(\frac{1}{p(p+1)(p+2)}-\frac{1}{(p+1)(p+2)(p+3)}\right)\right.$$
$$\left.-\left(\frac{1}{(p+1)(p+2)(p+3)}-\frac{1}{(p+2)(p+3)(p+4)}\right)\right]$$

$$=1.2.3p\left[\frac{1}{p\ldots(p+3)}-\frac{1}{(p+1)\ldots(p+4)}\right]$$

$$=\frac{1.2.3.4}{(p+1)(p+2)(p+3)(p+4)},$$ whether p is an integer or a fraction.

We may assume generally, if we do not object to fractional factorials,

the characteristic fraction for $\left(r^{\frac{1}{p}}-a^{\frac{1}{p}}\right)^q$ is $\dfrac{\lfloor q}{(p+1)\ldots(p+q)}=\dfrac{\lfloor p\,\lfloor q}{\lfloor p+q}$.

The reciprocal of this quantity is what Dr. Nunn gives in his tables. If we call P the power and R the root in these tables, the quantities are

$$\frac{\lfloor R+P}{\lfloor R\,\lfloor P}.$$

A. LODGE.

353. [V. 1. a.] *Conversion from Fahrenheit to Centigrade or vice versa.*

Add 40, multiply by $\frac{9}{5}$ths or $\frac{5}{9}$ths as the case may be, subtract 40. The graph is interesting.

J. P. KIRKMAN.

354. [X. 7.] *Note on the Use of the Protractor.*

Of the operations of geometrical drawing, the use of the protractor as commonly employed is the weak spot, so far as accuracy is concerned; the object of the present note is to draw attention to a method of using that instrument by which the operation of laying off an angle of a given number of degrees, and that of measuring an angle given on paper, can be carried out with accuracy at least equal to that attainable in the use of the compasses, the ruler and the set square.

The protractor, whether of circular or rectangular outline, should be complete, showing 360°, which should be marked round the outer edge of a thin piece of card or other material, having a large hole in the middle—a circular protractor might be, for instance, a circular annulus of 6 inches external and 4 inches internal diameter.

To lay off a line through a given point, making $n°$ with a given line (which may or may not pass through the point), proceed as follows. Place the protractor on the given line in such a way that the line extends beyond the protractor both ways, and bring the marks of 0° and 180° into coincidence with the line. Place a set-square on the top of the protractor so that one edge coincides with the marks of $n°$ and $180°+n°$ at opposite points of the

protractor's edge. Holding the set square in position, slip the protractor out from beneath it, and then, applying a straight edge to another edge of the set square, slide the latter till the edge first mentioned passes through the given point. Then draw the line.

To measure off the angle between two lines p and q (which may or may not meet), place the protractor so that the marks 0° and 180° coincide with the line p. Place a set square on the top of this with one edge, say A, coincident with the line q, then by the aid of another straight edge slide it so as to bring the edge A right across the protractor in such a way as to pass through two points on the latter's edge which differ by 180°. These points give the number of degrees in the two supplementary angles between p and q. R. F. MUIRHEAD.

355. [V.] The recently published catalogue of the Library of the American Mathematical Society seems to indicate that others beside the Editor of the *Mathematical Gazette* have been deceived by the titles of two French journals which contain practically nothing of interest to the mathematician. They are devoted to geodesy, topography, surveying, economic and legislative rural jurisprudence, etc. As a matter of record, and warning, it may be well to give their full titles, which are as follows:

I. *Journal des géomètres—organe officiel de la Société des géomètres-experts de France—fondée en* 1847. *Bulletin bi-mensuel du réglement et de la Conservation de la Propriété Foncière.* 1909. 62nd Year. VI. Series. Vol. XI.

II. *Journal des géomètres-experts—organe officiel de la Société nationale des géomètres de France, d'Algérie et de Tunisie. Revue bi-mensuelle.* 1909. 17th Year.

I may further add, as supplementary to the information on page 167 of the *Gazette,* (1) that the *Bulletin de Mathématiques Spéciales* was published for six years only—October 1894–July 1900; (2) that the price of *Revue de Mathématiques Spéciales* should be 11 fr. instead of 8s. R. C. ARCHIBALD.

356. [K. V. d.] *The Minimum Property of the Pedal Triangle.*

The usual proof of this property has the disadvantage of assuming the existence of an inscribed triangle of minimum perimeter. The following direct proof may be interesting if it is new.

Lemma. Let P and Q be two points on opposite sides of the line LM. Then the figure $LMPQ$ is equal to $\frac{1}{2}LM.PQ$ if LM and PQ are perpendicular; otherwise it is less than $\frac{1}{2}LM.PQ$.

I. Let ABC be an acute-angled triangle, O its circumcentre, and XYZ any inscribed triangle. Then

$$AO=BO=CO=R.$$

By the lemma, $\frac{1}{2}AO.YZ \geqslant AYOZ$, with two similar relations.

And, adding, we have $\frac{1}{2}R(YZ+ZX+XY) \geqslant \triangle ABC$,

the only case of equality being that in which AO, BO, CO are respectively perpendicular to YZ, ZX, XY: this is so only when XYZ is the pedal triangle.

It follows that the perimeter of the pedal triangle is $2\triangle/R$, and that any other inscribed triangle has a greater perimeter. Q.E.D.

II. If ABC is right-angled at A, there is no pedal triangle; but the argument just given shows that the perimeter of any inscribed triangle is greater than twice the perpendicular AP from A on BC. By putting X at P and allowing Y and Z to approach A, it is clear that the perimeter of XYZ will tend to $2AP$ as an inferior limit. There is no minimum.

III. The same is true if A is obtuse. Draw AC' perpendicular to AB. Then the perimeter of XYZ is greater than that of $XY'Z$, which is greater than $2AP$. The rest follows as in II. (If X lies between C and C', we draw AB' perpendicular to AC.) W. F. EGAN.

357. [I. 18. c.] *Powers of numbers whose sum is the same power of some number.*

Lest the readers of the *Gazette* who have not seen my publications on the subject might think Dr. Barbette the *discoverer* of the set of biquadrate numbers

$$4^4+6^4+8^4+9^4+14^4=15^4,$$

and of the set of fifth powers

$$4^5+5^5+6^5+7^5+9^5+11^5=12^5,$$

given in the review of his book on p. 95, No. 92 (May, 1911), it may be well to give briefly here a statement of the facts in connection with the history of these sets.

The set of biquadrate numbers whose sum is a biquadrate, given above, was communicated by the late Dr. David S. Hart, of Stonington, Conn., and given by me, with other sets, in a paper, "Methods of finding n^{th}-power Numbers whose Sum is an n^{th} Power," read at a meeting of the Mathematical Section of the Philosophical Society of Washington, Nov. 3, 1887, an abstract of which was published in the Bulletin of the Society, Vol. X., pp. 107–110, of the *Proceedings of the Mathematical Section*, thus antedating Dr. Barbette by 23 years!

The set of fifth-power numbers whose sum is a fifth power, given above, was discovered by the writer in Nov. 1887, and was given in the paper mentioned above 23 years before the appearance of Dr. Barbette's book. I also discovered, and gave in the same paper, the following set:

$$5^5+10^5+11^5+16^5+19^5+29^5=30^5,$$

which Dr. Barbette failed to "discover."

I also found

$$4^5+5^5+6^5+7^5+8^5+9^5+10^5+11^5+14^5+18^5+22^5=24^5,$$

and many other sets.

Dr. Barbette employs in his book the same tentative method, and practically the same notation that I did in my work done in 1887.

I published a paper, "About Biquadrate Numbers whose Sum is a Biquadrate," in the *Mathematical Magazine*, Vol. II., No. 10 (January 1896, Washington, D.C.), pp. 173–184. In that paper I found the following sets of five biquadrates whose sum is a biquadrate, which had also been communicated by Dr. Hart:

$$1^4+2^4+12^4+24^4+44^4=45^4,$$
$$4^4+8^4+13^4+28^4+54^4=55^4,$$
$$1^4+8^4+12^4+32^4+64^4=65^4.$$

Dr. Barbette finds only *one* set of five biquadrate numbers whose sum is a biquadrate number.

I published in the *Mathematical Magazine*, Vol. II., No. 12, pp. 285–296, a paper, "On Biquadrate Numbers," by Mr. Cyrus B. Haldeman, of Ross, Butler Co., Ohio, in which he finds by rigorous methods of solution many different sets of 5, 6, 7, 8, 9, etc., biquadrates whose sum is a biquadrate.

I contributed to the International Congress of Mathematicians held at Paris, August 9–12, 1900, a paper on biquadrates whose sum is a biquadrate, which was published in the *Proceedings of the Congress*, pp. 239–248, and later, with alterations, additions, and corrections, republished in Vol. II., No. 12, of the *Mathematical Magazine*, pp. 325–352, in which are found by rigorous formulas a great number of sets of biquadrate numbers whose sum is a biquadrate.

To the International Mathematical Congress held in connection with the World's Columbian Exposition, at Chicago, August 1893, I contributed a paper, "On Fifth-Power Numbers whose Sum is a Fifth Power," which was published in the *Congress Mathematical Papers*, Vol. I., pp. 168–174.

I have also found sixth-power numbers whose sum is a sixth power, discovering the first two sets August 31, 1891, and communicated them in a paper, "On Powers of Numbers whose Sum is the Same Power of Some Number," to the New York Mathematical Society, Oct. 3 of the same year, which paper was published in the *Quarterly Journal of Pure and Applied Mathematics*, Vol. XXVI. (London, 1893), pp. 225–227.

As they may be of interest to some readers of the *Gazette*, I will reproduce the two sets of six-power numbers here:

$$1^6+2^6+4^6+5^6+6^6+7^6+9^6+12^6+13^6+15^6$$
$$+16^6+18^6+20^6+21^6+22^6+23^6=28^6,$$

sixteen sixth-power numbers whose sum is a sixth power;

$$3^6+6^6+12^6+14^6+15^6+18^6+21^6+27^6+28^6+36^6+39^6+45^6$$
$$+48^6+54^6+56^6+60^6+63^6+66^6+69^6+70^6+98^6+126^6$$
$$+168^6+182^6+210^6+224^6+252^6+280^6+294^6+308^6+322^6=392^6,$$

thirty-one sixth-power numbers whose sum is a sixth power.

The writer is not aware that any person besides himself ever discovered any fifth-power numbers whose sum is a fifth power before the publication of Dr. Barbette's book, and he is not aware that *any* person but himself has ever discovered sixth-power numbers whose sum is a sixth power.

Dr. Barbette does not seem to be aware that any other mathematician had discovered these fourth-power and fifth-power numbers before he found them. It is certainly remarkable that he had not seen any of my papers, as they have been so widely disseminated. ARTEMAS MARTIN.

QUERIES.

(74) If the altitude of an isosceles triangle is equal to the base, prove that the perpendicular from the vertex on the base is divided in median section by the inscribed circle. Is this relation known? G. E. C. CASEY.

(75) From X, Z parallels XA, ZC are drawn. AZ and CX intersect in B. A parallel through B to AX or CZ cuts XZ in Y. Shew that

$$XA^{-1}+ZC^{-1}=YB^{-1}.$$

Has this been developed? It can be utilised in electricity, optics, etc.

W. R. BOWER.

ANSWER TO QUERY.

[**63, p. 119, vol. v.**] Goodman's Planimeter is a Hatchet Planimeter with an adjustable arm which can be set to the length of any figure (*e.g.* an indicator diagram), and thus the average height can be determined.

The mathematical theory of the instrument was given by the inventor Captain Prytz in *Engineering*, June 22nd, 1894, and another on similar lines was given by F. W. Hill in a paper read by him before the Physical Society and published in the *Philosophical Magazine*, Vol. 38, 1894.

There was also given in *Engineering*, May 25th, 1894, a geometrical proof, and another was given by Prof. Henrici in the *B.A. Report*, 1894, and in the article by him in the *Ency. Britt.* on "Mathematical Instruments."

J. A. TOMKINS.

ERRATA.

Pp. 128, 9. For fig. 2 read fig. 4; for fig. 3 read fig. 2; for fig. 4 read fig. 3.

THE LIBRARY.

THE Library is shortly to find a home in the rooms of the Teachers' Guild. A catalogue will be issued to members in due course, containing the list of books, etc., belonging to the Association and the regulations under which they may be inspected or borrowed.

The Librarian will gladly receive and acknowledge in the *Gazette* any donation of ancient or modern works on mathematical subjects.

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REVIEWS.

Text-book on Practical Astronomy. By G. L. HOSMER. 8vo. Pp. 210. \$2.00 net. 1910. (New York, John Wiley & Sons; London, Chapman & Hall.)

The author of this work is the Assistant Professor of Civil Engineering in the Massachusetts Institute of Technology, and his intention has been to provide a text-book useful to students in his own faculty.

The plan is adapted to this end. First, we have definitions of the celestial sphere, zenith, meridian, ecliptic, right ascension, and so forth, and such formulae of spherical trigonometry as will be required in the subsequent calculations. The author then considers sidereal and mean time, and the equation of time, and explains the use of the ephemeris. After this follows an account of the earth's figure and of those corrections, *e.g.* parallax and refraction, which depend on terrestrial circumstances. A chapter is then devoted to the instruments used in astronomical observations by surveyors, *e.g.* the engineer's transit; and after a short digression on the constellations, we come to the main body of the work—an account of the methods of determining practically the latitude, time, longitude, and azimuth.

The work is well planned, and some of its features are excellent—for example, the abundance of figures, many of them admirably drawn. Its faults resolve themselves for the most part into carelessness, manifested in different ways. There are many blunders in the diagrams—for instance, in Fig. 13 the letter C is used twice over; in Fig. 50 two different stars are marked γ Eridani, and the indicated brightness of Bellatrix is too low by three magnitudes, of Castor and of δ Canis Majoris too high by one magnitude, of γ Geminorum too low by one magnitude, and of ζ Orionis, and of β and η Canis Majoris too low by two magnitudes. In the early chapters there are many loose statements, such as "it is evident that the hour angles of all points on the celestial sphere are always increasing" (p. 21), and "Mars, Jupiter, and Saturn are outside the earth's orbit, and therefore revolve around the earth" (p. 102); the literary style is poor throughout. Many of the definitions given are unnecessary; for instance, almucantars, equinoctial and solstitial colures, and the parallactic angle (used of the spherical angle subtended by the pole and zenith at a point on the celestial sphere). Lastly, the author sometimes wraps obvious facts in technical language and then presents them as interesting results, as when (p. 29) we are told that "if a person were at the earth's pole," "the longitude of a point on the earth and its azimuth from the Greenwich meridian would then be the same."

In spite of such defects as these, we believe that the book will prove useful, and will have a considerable circulation; and if it should be carefully revised when a second edition is called for, there is no reason why it should not rank among the works indispensable to the professional student. E. T. WHITTAKER.

Wahrscheinlichkeitsrechnung. Vol. II. **Mathematische Statistik, Mathematische Grundlagen der Lebensversicherung.** By EMANUEL CZUBER. Second, revised and enlarged, edition. Pp. x+470. 8vo. 1910. (Teubner, Leipzig.)

Professor Czuber's first edition, published in 1903, was in only one volume. By reserving for this second volume some of the principal applications of the fundamental theorems dealt with in the first, he is now able to introduce a considerable number of additions. Almost all the changes which Professor Czuber has made are by way of addition, and he has retained without alteration almost the whole text of the first edition. The most considerable of these enlargements are directed to a fuller treatment of the subject of invalidity. There are new sections on invalidity statistics, on invalidity insurance, and on State insurance.

The mathematical theory of mortality statistics and of life insurance, the subject of invalidity being introduced as matter subsidiary to this, occupies the greater part of the volume now under review. So far as I can judge, these topics have been admirably treated, and the very numerous references to other writers ought to render it exceedingly valuable to students of the technical detail of this subject. But with technical detail it is mainly concerned, and there is not a great deal of much interest to mathematicians as such or to general students of the theory of statistics in relation to probability. This does not apply, however, to the first section of the first part, entitled *Die menschlichen Massenerscheinungen*, in which there is a good deal of general theory relating to statistical or inductive probabilities, and in which he supplies a very useful account of the mathematical basis of the statistical methods associated with the name of Prof. Lexis. Probabilities, which are based upon evidence arising out of observed statistical frequencies, Professor Czuber usefully distinguishes under the name of *statistical* probabilities from those, such as occur in the discussion of games of chance, arising out of a number of alternatives which the principle of non-sufficient reason permits us to regard as equi-probable. Under this heading he deals, by methods very much like those adopted by numerous other writers, with such questions as the probable limits of unobserved statistical frequencies, given the values of certain similar statistical frequencies which have been observed in the past. I am inclined to think that almost the whole of this is not, as it stands, altogether valid. The probability of an induction cannot be calculated by these precise numerical methods, and some of the conclusions to which they lead seem plainly contrary to what it is reasonable to believe. In order to employ these methods, Professor Czuber has, in fact, to rely upon veiled appeals to the principle of non-sufficient reason at points of the argument, when he is not, apparently, aware that he is doing so, and at points where appeals to this principle cannot be made legitimately. It is the fundamental error of this part of the classical theory of probability that it arrives at far too high a probability in favour of *any* hypothesis that it is called in to support.

J. M. KEYNES.

The Dynamical Theory of Sound. By HORACE LAMB, F.R.S. 1910. (Edward Arnold.)

This new book by Prof. Lamb will add to his high reputation as a writer on applied mathematics. The ground covered is very nearly the same as that of Lord Rayleigh's treatise, but the investigations are not worked out in quite so much detail. For example, symmetrical spherical waves in air are discussed in full, but the unsymmetrical waves, the theory of which would involve spherical harmonics and Bessel functions of order $n+\frac{1}{2}$ are dismissed with a reference to Lord Rayleigh. As a general rule all questions of practical interest are treated at sufficient length, although we note that singing flames are dismissed very briefly. This is less to be regretted as the theory is to be found in Thomson and Poynting's *Physics*. In discussing scales Prof. Lamb states that "the requirements of most keys can be fairly well met" by the system of equal temperament. We should have thought that all keys were on the same footing, and that the prejudice that some musicians have in favour of certain keys had no more mathematical justification than the prejudice of some whist players in favour of certain suits; but the physiological side of the subject of absolute pitch is so obscure that it would be unwise to dogmatise on this point. Of course failures in consonance are more important in the slow music of the organ with its sustained notes than in the

rapid music of the piano. The explanation of equal temperament should surely be accompanied by a reference to the fact that it is attained in practice with sufficient accuracy by tuning the fifths and octaves correctly.

The subject of sound has been worked out with such thoroughness by Helmholtz and Lord Rayleigh that the new work is naturally confined to details. Amongst the references to recent work quoted by Prof. Lamb we note the experiments of Krîgar-Menzel and Raps, who found that a string plucked at one point actually passes through the shapes predicted by theory, and the investigations by Lord Rayleigh which show that our perception of the direction of a sound depends on the difference of phase between the waves which reach our two ears. It is hardly necessary to add that in our opinion this treatise will prove a most valuable addition to the library of every student of mathematical physics. F. J. W. W.

The Elements of the Theory of Algebraic Numbers. By L. W. Reid. Pp. 454. (New York: Macmillan.) 1910.

Prof. Reid's object can best be stated in his own words in the preface: "It has been my endeavour to lead by easy stages a reader, entirely unacquainted with the subject, to an appreciation of some of the fundamental conceptions in the general theory of algebraic numbers. With this object in view, I have treated the theory of rational integers more in the manner of the general theory than is usual, and have emphasised those properties of these integers which find their analogues in the general theory.... The theorems and their proofs have therefore been so formulated as to be readily extendable in most cases to the general realm of the nth degree, and it is hoped that a student who wishes to continue the study of the subject, will find the reading of works on the general theory, such as Hilbert's *Bericht über die Theorie der Algebraischen Zahlkörper*, rendered easier thereby."

The only other book on similar lines is Dr. J. Sommer's *Vorlesungen über Zahlentheorie* (Leipzig, 1907), and a comparison between the methods of these two authors may be interesting. Reid's book is twice as thick as Sommer's, but Sommer's is more closely printed, and probably contains nearly the same quantity of print. Reid is occupied up to page 155 with rational numbers, and then treats in turn algebraic numbers of the forms

$$x+yi, \quad x+y\sqrt{(-3)}, \quad x+y\sqrt{2}, \quad x+y\sqrt{(-5)},$$

and ends with some chapters on the quadratic realm in general. Sommer plunges at once into the quadratic realm, and treats also of the cubic realm, and of a species of realms of the fourth degree. Reid's book resembles French mathematical books in its clear and easy style, while Sommer's book is in the more solid and condensed style of the Germans. There can be no doubt that Reid's easy stages are better for the beginner, and the many numerical examples fully worked out will also be helpful to him. It is a pity that Prof. Reid could not find room for the genera in quadratic realms, having regard to the great importance genera possess in the more advanced theory, but still a line had to be drawn somewhere. A list of additional Errata has been published, and any purchaser of the book, who has not got this list, may obtain it from Messrs. Macmillan & Co.

A. E. Western.

Exercises from Algebra for Secondary Schools. By Charles Davidson. Pp. vi. + 320. 1911. (Cambridge: Univ. Press.)

This book is a reprint of the examples from Dr. Davidson's well-known algebra for secondary schools, and as such needs little comment. The author and his publishers have done their work excellently, and the book will be found useful in those cases where it is thought better not to place the usual subject matter in the hands of the pupil. Exercises on the exponential and logarithmic series and on convergency of series are included, as well as many good sets of miscellaneous examples. H. G. M.

Algebra (Part II.) By K. J. Chottoraj. Pp. 486. 1910. (Chottoraj.)

This volume is intended to meet the requirements of students preparing for the Intermediate and Previous Examinations of Indian Universities under the New Regulations, and covers the second half of the ground usually taken-up to the Exponential and Logarithmic Series. Though errata are numerous and the

general printing and arrangement of the book are not of the best, the treatment of the subject is commendable. In a great many cases the method of attack and mode of expression are original, and the book should be productive of ideas for the teacher. The private student, too, will receive many practical hints, especially in the work on the Binomial Theorem and on Logarithms from the arithmetical point of view. The exercises have clearly received much care and attention from the author, and are one of the features of the book, whilst many of the worked-out examples in the text have been well chosen from the various Indian University papers. H. G. M.

Elements of Mechanics. By G. W. PARKES. 4s. 6d. 1911. (Longmans, Green and Co.)

It is difficult to understand what useful purpose can be served by publishing a work which is so very much on the old lines. Both parts, Statics and Dynamics, are almost entirely theoretical, and except for an experimental verification of the Parallelogram of Forces there is scarcely an allusion to the experimental work which can be done by the student. It is said of Lord Kelvin that when lecturing on Mechanics he used to tell his students that Levers had been divided into three classes, but that he could not tell them what they were, and if further information was required they would have to refer to textbooks. We have here, in addition to the three classes of Levers, the three systems of Pulleys supplemented by a fourth, and the only machines described are the Common or Roman Steelyard, the Danish Steelyard, the Wheel and Axle, the Differential Wheel and Axle, the Inclined Plane, the Wedge, and the Screw.

The dynamical part of the book would have been greatly improved by a chapter on the Dynamics of Rotation of a Rigid Body. Reference is made to the relativity of Potential Energy, but none to the Relativity of Velocity. No mention is made of the loss of Kinetic Energy on the impact of two Bodies, and there is no allusion, in the treatment of Projectiles, to the important part played in practice by the resistance of the air.

The figures are, as a rule, only diagrammatic, and in some cases the drawing seems defective. In Fig. 78, p. 122, it is difficult to make out whether a perspective sketch or a projection is intended. The figure given corresponds to neither. The projection of a spiral or screw thread on a plane parallel to the axis is of course a sine curve.

The strong feature of the book is the insistence on the use of first principles in working examples. Many good typical examples are worked out in the text.

R. M. MILNE.

(1) **Allgemeine Theorie der Raumkurven und Flächen.** By DR. V. KOMMERELL und PROF. K. KOMMERELL. II. Band. Pp. 188.

(2) **Spezielle Flächen und Theorie der Strahlensysteme.** By the same authors. Pp. 171.

These are respectively Nos. 44 and 62 of Schubert's set of mathematical text books (Leipzig). They form with a preceding number (29) a complete work on Differential Geometry. Apparently No. 44 in the first edition contained matter which is now divided into these two books, Nos. 44 and 62, which have been published as the second edition of Band II. of the original treatise with considerable additions. Notably there are many more examples introduced to illustrate the general theory, and a section on Congruences has been appended.

In No. 44 the line followed is Gauss's method of dealing with a surface in terms of two parameters. All the general properties receive treatment with remarkable completeness considering the size of the book. The argument, of course, is that at parts of a surface where it is approximately flat (as opposed to nodes or discontinuities) the element of arc is expressible as

$$ds^2 = E\,du^2 + 2F\,du\,dv + G\,dv^2,$$

and the asymptotic lines are given by

$$L\,du^2 + 2M\,du\,dv + N\,dv^2 = 0,$$

where E, F, G, L, M, N are functions of u and v, such that the shape and size of the surface is unique if these six functions are given. This is analogous to the theorem that a plane curve is intrinsically given if the curvature is a known function of the arc.

From this point of view most naturally follow investigations of the lines of curvature, minimal lines, geodesics of a surface, and problems connected with the representation of one surface on another. The treatment of the latter, and in particular the spherical representation of one surface, are especially good.

In No. 62 the special surfaces are first of all W-surfaces, which are defined by a relation existing between the two principal curvatures at every point. The prominent examples are minimal surfaces ($\rho_1+\rho_2=0$), and surfaces of constant total curvature ($\rho_1\rho_2=$constant). Next come ruled surfaces, and lastly triply orthogonal systems of surfaces. Considering the amount known on minimal surfaces, the short reference to them is remarkably complete. The more purely geometrical enquiry is omitted as well as that interesting Problem of Plateau, viz. to find the shape taken by a film with a given rigid closed boundary. The section on surfaces of constant total curvature makes an interesting introduction to non-Euclidean geometry: there is quite an instructive reference to the trigonometry of a pseudo-sphere. The last part of the book, on Congruences, is intended to familiarize the reader with the most recent point of view and some of its results. It deals with a surface as one of the family of surfaces with the same normal congruence. The element is the straight line, and its position is governed by two parameters. The work falls into a general theory and special cases for W-surfaces, and so on. This is perhaps the most valuable part of the whole work.

H. W. TURNBULL.

Vorlesungen zur Entführung in die Mechanik raumerfullender Massen. By A. BRILL, Professor of Mathematics in the University of Tübingen. Pp. 236. Price 8 marks. 1911. (Teubner.)

This book has a twofold design, to serve as an introduction to the mathematical treatment of the mechanics of a continuous medium, and to employ for that purpose the ideas set forth by Hertz in his *Mechanics*.

Much interest has always been felt in various "economic" statements of the laws of nature, and some day an interesting monograph will doubtless fill up the outline sketched by Mach, tracing back the principle that "Nature always acts by the simplest and most perfect means" to the schoolmen if not to Aristotle, setting forth the first triumph of the principle in suggesting to Fermat the famous law of swiftest propagation of light, and not omitting Maupertuis and the "principle of least action."

Fermat himself noticed the difficulty that in certain cases the time is not a minimum but a maximum, a result now expressed by saying that the time has a stationary value. Mach observed that any equation of motion may be regarded as the condition that some function has a maximum or minimum value. But the attraction of the notion remains, and many illustrious mathematicians have endeavoured to state an equivalent to Newton's laws in this economic form. Gauss stated his principle of least constraint thus:

Define the constraint of a particle of mass M as $M\xi^2$, where ξ is the distance between its actual position after a small time and the position it would have taken up in the same time if free from all constraints. Then $\Sigma(M\xi^2)$ is a minimum. As a simple example, consider M hanging vertically, dragging m along a smooth table.

If the acceleration is f, the constraints are

$$M\,.\,(\tfrac{1}{2}gt^2-\tfrac{1}{2}ft^2)^2 \text{ and } m\,.\,(\tfrac{1}{2}ft^2)^2,$$

so $mf^2+M(g-f)^2$ is to be a minimum, whence $mf-M(g-f)=0$; giving the usual result.

Hertz, desiring to get rid of action at a distance and of "potential energy" as a mechanically unexplained phenomenon—postulated invisible "concealed" latent or hidden masses in motion as sources of energy, and their kinetic reactions as sources of force.

A simple example may be constructed by thinking of the dynamics of a race, living on the inner surface of a hollow sphere rotating with large angular velocity about a diameter, and unconscious of this movement.

Hertz formulated his version thus: Every system subject only to internal constraints independent of the time moves in the straightest possible path, *i.e.* so that the "curvature" is a minimum.

The curvature is defined as $\Sigma m\left(\frac{d^2x}{ds^2}+\frac{d^2y}{ds^2}+\frac{d^2z}{ds^2}\right)$.

Professor Brill sets out the Hertzian system, and illustrates it by examples. It is curious to note that the example chosen of non-holonomous systems, in which the constraints are expressible by a differential equation unintegrable *per se*, is as simply dealt with by direct application of Newton's second law, which avoids what Professor Brill terms the "delicate considerations and special precautions" necessary in applying Lagrange's equations.

The book is divided into four sections, dealing with material particles and rigid bodies, the mechanics of fluids, of elastic solids, and with the electromagnetic theory of light. It possesses high interest as indicating a *sequence* in the branches of mathematical physics discussed, for the co-ordination of a student's progress in the working of mathematical machinery, with his progress in familiarity with the physical problems to be treated, is a matter which is far from invariably being well regulated.

A Key to the Theory and Methods of Linear Perspective. By C. W. Dymond, F.S.A. (London: E. & F. Spon). Pp. 32, 8 plates. Price 1s. 6d.

This is a brief statement of the first principles of perspective illustrated by ten worked problems, with diagrams printed in three colours. The theory of measuring points is stated with unusual clearness, and the book should prove a useful introduction to the subject.

Engineering Descriptive Geometry. By F. W. Bartlett and T. W. Johnson. Pp. 159. 1910. (J. Wiley & Sons, New York; Chapman & Hall, London.)

In this book the elements of descriptive geometry are set forth—with special regard to the needs of the practical draughtsman. The most noteworthy features of the book are the diagrams, which are admirably clear and inspiriting. One query may be submitted: For what reason do the authors, presumably following American practice, depart from the invariable custom of England, France, and Germany, by placing the plan above the elevation? C. S. J.

Annuaire pour l'an 1911, publié par le Bureau des Longitudes avec des Notices Scientifiques. Pp. vii + 641 + 29 × 18 + 13 × 11 × 51. 1 fr. 50. 1911. (Gauthier-Villars.)

The current number of the *Annuaire* is devoted to tables dealing with Metrology, Moneys, Geography, Statistics, and Meteorology. Last year's and that of next will be given up to Chemical and Physical data, but will not deal with the subjects of the Tables in the present volume. Similarly the Astronomical sections contain Tables for the calculation of altitudes by the barometer, stellar parallaxes, double stars of known orbit, spectroscopic double stars, and proper motions. The paper on stellar spectroscopy has been entirely re-written by M. de Gramont. Whatever else is required by astronomers in the way of tables for their ordinary work will be found in the *Annuaire* for 1912. The Notices comprise Notes on the Sixteenth Conference of the International Geodetic Association by M. H. Poincaré, the Solar Eclipse of 1912 by M. G. Bigourdan, and funeral orations by M. Poincaré and Baillaud on the occasion of the obsequies of MM. Bouquet de la Grye and M. P. Gautier.

BOOKS, ETC., RECEIVED.

Lectures on the Fundamental Concepts of Algebra and Geometry. By J. W. Young. *Prepared for publication with the co-operation of* W. W. Denton, *with a Note on the Growth of Algebraic Symbolism,* by U. G. Mitchell. Pp. vii + 247. 7s. net. 1911. (Macmillan Company.)

Junior Algebra, with Answers. By W. G. Borchardt. Pp. viii + 317. 2s. 6d. 1911. (Rivington.)

Die Komplexen Veränderlichen und Ihre Funktionen, Fortsetzung der Grundzüge der Differential- und Integralrechnung, zugleich eine Einführung in die Funktionentheorie. By G. Kowalewski. Pp. 456. 12 m. 1911. (Teubner.)

Theorie der Linearen Differenzengleichungen. By A. Guldberg and G. Wallenberg. Pp. xiv + 288. 10 m. 1911. (Teubner.)

Lehrbuch der Differential- und Integralrechnung. By J. A. Serret. Edited by G. Scheffers. 5th edn. Vol. II. *Integralrechnung.* Pp. xiv + 638. 13 m. bd. 1911. (Teubner.)

Higher Mathematics for Chemical Students. By J. R. Partington. Pp. 272. 5s. 1911. (Methuen.)

Théorie des Fonctions Metasphériques. By Niels Nielsen. Pp. 212. 1911. (Gauthier-Villars.)

Graphical Representation of some of the Simpler Analytic Functions of a Complex Variable. By E. T. Littlewood. Pp. 173-182. Reprinted from *Trans. Roy. Soc. S. Africa.* Vol. II. Part II. 1911.

Sopra Alcune Formule Fondamentali nell' Analisi Spaziale ad n Dimensioni di Grassmann. By A. Del Re. Pp. 17. Reprinted from *Rend. della R. Accademia delle Scienze Fisiche e Matematiche di Napoli.* Mar. and Ap. 1911.

Optical Geometry of Motion, a new View of the Theory of Relativity. By A. A. Robb. Pp. 32. n.p. 1911. (Heffer, Cambridge.)

Horner's Method anticipated by Ruffini. By Florian Cajori. Pp. 409-414. Reprint from the *Bulletin of the American Math. Soc.* Vol. XVII. No. 8. 1911.

Introduction to Mathematics. By A. N. Whitehead, F.R.S. Pp. 254. 1s. net. 1911. (Williams & Norgate.)

On Fifth Power Numbers whose Sum is a Fifth Power, 1893; *About Cube Numbers whose Sum is a Cube Number,* 1896; *About Fifth Power Numbers whose Sum is a Fifth Power Number,* 1898; *A Method of Computing the Common Logarithm of a Number without making use of any Logarithm but that of some Power of Ten,* 1900; *About Sixth Power Numbers whose Sum is a Sixth Power,* 1903. By Dr. Artemas Martin. *About Biquadrate Numbers whose Sum is a Biquadrate.* By Dr. Artemas Martin and Cyrus B. Haldeman. 1910. (Washington.)

The Theory of Groups of Finite Order. By W. Burnside. 2nd edition. Pp. xxiv. 15s. net. 1911. (Cambridge University Press.)

Report of a Conference on the Teaching of Arithmetic in London Elementary Schools. Dec. 1906-1908. Pp. 134. Published May 30, 1911. 1s. (King & Son.)

Memorandum on the Teaching of Elementary Mathematics. By Prof. H. S. Carslaw. *Issued by the Department of Public Instruction of New South Wales.* Pp. 31.

The Bolyai-Lobatschewsky Non-Euclidean Geometry: an Elementary Interpretation of this Geometry, and some Results which follow from this Interpretation. By Prof. H. S. Carslaw. *Extracted from the Proceedings of the Edinburgh Mathematical Society.* Vol. XXVIII. 1909-1910.

Elementary Applied Mechanics. By A. Morley and W. Inchley. Pp. viii + 382. 3s. net. 1911. (Longmans, Green.)

Algebraische Kurven. By E. Beutel. Part II. *Theorie und Kurven dritter und vierter Ordnung.* Pp. 136. 80 pf. Sammlung Göschen. No. 436. 1911. (Göschen, Leipzig.)

On the Foundation and Technic of Arithmetic. By Prof. G. B. Halsted. "The Open Court." April and May, 1911.

Bibliography of Works of Dr. G. B. Halsted relating to Non-Euclidean Geometry and Hyperspace. By D. M. Y. Sommerville. (To be published by Messrs. Harrison.)

Berichte und Mitteilungen, veranlasst durch die Internationale Mathematische Unterrichtskommission. V. Pp. 55-74. VI. Pp. 75-88. 60 m. 1911. (Teubner.)

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